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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Factors influencing the large-scale distribution of Hg° in the Mexico City area and over the North Pacific" *by* R. Talbot et al.

R. Talbot et al.

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This was a very thoughtful review with good suggestions. We thank the reviewer for them.

Our paper is really the first look at Hg° on a large scale over the North Pacific. There are few data to compare to, and the relevant ones are mentioned in the text. The most relevant data are from Friedli et al., and we compared to their data for what we think are the most interesting features in the Hg° distribution. We are of the opinion that we do not know enough about Hg° to make comparisons to other seasons and locations.

We mentioned that the low mixing ratios of Hg° at 2-6 km around the Mexico City area might be related to anthropogenic halogen emissions (p. 16). Since these air masses were aged (low CO), we added the possibility of slow oxidation in the Tropics. As

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suggested by the reviewer, we moved this to Section 3.1.

We are not sure what else can be said from the few data collected in the plumes. We showed that Hg° was correlated with CO and other tracers in these urban plumes (Figure 7), so there are significant urban-industrial sources of Hg° around Mexico City. In the boundary layer ($<$2 km) the Hg° data were identical at all three locations (p. 10). Although there is an oceanic source, it is not visible in our mixing ratio measurements. The flux would have to be measured to determine the source strength.

We believe that adding to the text would bring even more speculation into the manuscript. We simply don‘t know what is responsible for many aspects of the Hg° distribution.

In the Tropics the air is generally moving vertically, so that boundary layer air gets mixed upward. We were anticipating that Hg° might be reduced in the marine boundary layer due to its oxidation by halogens. There are so few data for Hg° and RGM over the ocean we don‘t know what the distribution looks like, the amount of oxidation over the open ocean, or really what to expect. This is especially true for the Tropics. We modified the text to include what we were thinking.

We added a paragraph to compare the altitudinal distributions of Hg° over the Pacific.

We removed the paragraph on depletion in the marine boundary layer.

We added a paragraph to mention the low Hg° found at low altitude, although we could not decipher the reason for it.

We added the Hg°/CO values determined from figure 9.

We added the r^2 and p values.

A sentence was added to try and clarify the trends versus latitude in CO_2 were opposite of what is expected.

We looked in detail at the low values at low altitude and can not decipher their source.

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The stratospheric signal is lost (O_3 and potential vorticity).

We moved this section as suggested.

We added a reference to Figs 4 and 7.

We are not sure if the stratospheric impact on Hg° is present at the surface at the study locations.

We believe that the last sentence in this section qualifies the sentence previous to it.

We edited the conclusion section.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15533, 2007.

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